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A DISCUSSION OF A NEW FORMULA

FOR THE

FLOW OF WATER IN OPEN CHANNELS

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ROBERT GORDON

MILANO
TIPOGRAFIA E LITOGRAFIA DEGLI INGEGNERI
Via Lupetta Num. 7 e 9.

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PREFATORY

Several questions having arisen with respect to the engineering results of the Embankment Works in progress on the Irrawaddie River, the undersigned received orders to report on the various points raised. To do this effectually it was thought necessary both to take extensive observations on the river, and to study in the works of various writers on hydraulics the state of the science, and the views held regarding those points.

It was hoped that a brief review of those opinions, with short references to the maxims and established positions of the science, would permit a concise mode of dealing with the observations which were being accumulated respecting the river; but a very short study showed that in all branches of river hydraulics, not only are conflicting views held regarding what should be the fundamental parts of the science, but in some cases the most opposite practices have been carried into effect in contiguous localities.

At the very threshold of the science, the practical hydraulician is met by a strange confusion of formulas; none of those in use being capable of general acceptation, and none being theoretically derived; while the theoretical writers fail totally in their efforts to connect practise with theory, but succeed thoroughly in showing each other wrong.

The embankment works on the Irrawaddie have up to the present time been carried out exclusively in the Deltas on the right bank of the main river, and on the east bank of its westermost effluent, the Bassein River. Only about 150 miles of embankment have as yet been executed; but the works now progress at the rate of 30 to 40 miles per year from the head of the Delta sea-wards. A great change in the regime of the river has already resulted; and as a

proposal to embank the east bank in the same part was submitted to the Government of India, it became necessary to attempt to estimate the effect of shutting in to the main channel of the river all the water which now yearly inundates to depths of 30 feet, the neighbouring low lands.

Current measurement were established at the head of the Delta, below all important influents; at 110 miles lower down on the main bank, where a large quantity of water is lost; and on the right effluent-the Bassien river. The measurements were executed on the system used by Humphreys and Abbot on the Mississippi; the subsurface velocities being regularly taken, 10 in number, with floats at every metre in depth until the bottom was reached. From 30 to 60 surface velocities were also taken. The results so far are insufficient to report upon, and the observations will be carried on for another year; but most interesting curves have been obtained from the subsurface velocities, which fully establish the suggestion of Guglielmini, and the doubt of Prony, that no general relation independent of the surrounding conditions, can be found between, the maximum, the mean, and other velocities. This relation varies with every change of condition.

To discuss the observations made for discharge and to apply the results to other sections of the river, it was necessary to select a formula for estimating the discharge. It was immediately obvious that none of the formulae published would apply without alteration, and it was difficult to find the grounds on which a rational alteration could be made.

It was hoped at first to make an eclectie formula from those which have received tho support of names eminent in the science; but a closer study showed that this was impossible with any pretence to theoretical accuracy.

The total failure of Dubuats theory in the hands of most eminent mathematicians, and the absolutely empirical character of all the practical formulae, suggest the possibility of accounting for the flow of water in open channels by the abandoned theory of the Italian school; and the accompanying paper is an attempt to do this. The paper has been roughly put together, merely to enable criticism to be brought to bear on the points raised. The formula offered is rather a suggestion to be followed out at leisure, than one aiming at finality.

As the paper is being printed by the Local Government of Burma for circulation and criticism in the Public Works Department of India, the liberty is taken of sending a few copies to gentlemen interested or eminent in the science. It is particularly desired that no publication be made of the paper wholly; though no objection exist to abstracts or criticism, if it be thought with while to make any.

As the eminent Italian Engineer Signor Commendatore Lombardini, has shewn much kindness in guiding to, and enabling the requisite books to be obtained, the honor has been sought of dedicating the new formula to him. He has added to his kindness by permitting this. Though fully sensible of the danger attending an attempt of this kind, and of the blame which must naturally follow should it be found on due criticism that the attempt has been made on insufficient grounds; this honor is now availed of, and in the connection of the illustrious name of Lombardini with the new formula, is to be found only a new proof of his desire to assist and hold out encouragement to every earnest student of the science; while the responsibility remains with the undersigned.

Milan, 4 August 1873.

ROBERT GORDON.

FRAGMENT OF A REPORT *to be submitted on the Irrawaddy Embankment Works, containing a discussion on a proposed new formula.*

200. The knowledge of the quantity of water flowing through a channel of given dimensions in a given time is the first and indispensable preliminary before the hydraulic engineer can suit works of practical utility, depending on that flow, to the purpose for which they are intended. Some method of acquiring this knowledge approximately must have been in use in the earlier ages, in order to have permitted the great works then executed to answer their ends so admirably as history records them to have done; but it was only during the last few centuries that efforts to combine theory with practice to furnish concise rules, at once rational and practical, for estimating the flow from data ordinarily available, became general. These rules have varied much, and often give widely different results; and though the importance of the matter has increased with the magnitude of the interests involved, and a continually increasing attention and study has been given to the subject, no rules or formulae hitherto promulgated have received general assent, or acquired finality of form. The general dissatisfaction is evinced by the great number of formulae in the field, differing from each other not only in their numerical coefficients, but in the degrees in which the differents factors enter. The divergence has become wider during the last few years; and though there appears to be an agreement amongst their various promoters, as to the theory on which they are based, no attempt is now made at a rigorous deduction from theory of the formulae; all being based on purely empirical data. An entire agreement, however, is shown as to the value of the experimental data upon which they are grounded; and it is perhaps because the number of these has increased greatly, as well as their comparitive trustworthiness, during the last few years, that the number and variety of the formulae have also increased.

201. The divergences, then, can not depend on the published experiments, but may arise from errors in the theory on which they are based. An examination of this theory may show what grounds there are for this conjecture.

202. Three theories have at differents times been held respecting the flow of water in open channels. They vere based respectively on the principle of Castelli, the principle of Torricelli, and the principle of Dubuat.

203. Signor Lombardini in his work « Dell' origine e del Progresso della Scienza Idraulica » gives an interesting account of the history of the principle of Castelli. From 1628 to 1642, various works of his on hydraulic subjects were published. In the Treatise on the measurement of running waters (Book II, Prop II) he says: « If the water of a river, or of an aqueduct, move with a certain velocity though a regulator, having a given height, and then by the addition of new water this height is doubled, the velocity also will be doubled (1). »

204. He then shows that the pressure of the water will be doubled with double the height, and infers that the velocity will be as the height. He shows that this being the case, the scale of velocities, that is, the graphical representation of the different velocities from the top to the bottom, would be a triangle with the vertex uppermost, the maximum velocity being lowest. His pupil Bonaventura Cavaliere had given a similar account, but said that as the upper waters would be carried along by the lower, the scale of velocities would be represented by a triangle with the vertex downwards, the maximum velocity being at the surface (2).

205. The principle being that the velocity increased with the pressure, and this with the height, the theory founded on it was that discharge would be as the square of the height.

206. This theory was the first attempt to constitute a rational science of hydraulics. It connected the velocity of the flow, and thus the discharge, with the dimensions of the channel, and though the principle on which it was based afterwards proved to be erroneous, the originator of the principlo has deservedly come to be acknowledged as the founder of the science of hydraulics.

207. Lombardini shows that while this honor has been generally given to Castelli, it really belongs to Leonardo da Vinci, who a century and a half before, had given a complete statement of the principle and theory in writing. His manuscript works had been neglected and scattered; some having been taken to Spain, and others to France in 1797. Venturi examined those in Paris in that year, and found the above principle fully stated, together with numerous other valuable material relating to hydraulics, and other physical sciences. Subseqently, in 1825, was published a Treatise on the motion and measurement of water, extracted from the work of Leonardo da Vinci in 1643, by a Domenican Friar, L. M. Arconati, and deposited by him in the Barberini Library, Rome. This contains propositions very similar to those of Castelli; and Lombardini argues that Castelli, who was consulting hydraulician to Pope Urban VIII, Barberini, must have had access to Leonardos' manuscripts, and taken his principle from them.

(1) Lombardini, *Dell'origine* ecc., pag. 55.
(2) Idem, pag. 51.

208. Lombardini examines the position of the science in the different States of North Italy, in the interval between Leonardos' death, and the publication of the work of Castelli.

209. In Romagna and the Ferrarese, although no method was known of calculating the quantity of water, the value of the velocity of this was known. In Venetia also the velocity of the water was appreciated. In Lombardy, until 1566, no record appears of rational calculations of the effect of the velocity of water on the discharge; but after that date, not only was this appreciated, but the principle above given was used, and is found distinctly stated in a manuscripti of Betinzoli, about 1600. Lombardini surmises that after the dispersion of Leonardos' writings « the engineers took extracts to enrich their manuscripts, which at that time constituted the Code of their art, founded mostly on empiricism; manuscripts which were kept with mysterious secrecy, to be transmitted to their proper successors. Just then sprang up many distinguished engineers, such as Meda, Bassi, Pellignini, Pirovano, Lonati, and lastly Burti, who all, it would appear, drew from that fount, from which also the engineers of other dominions, profitted, amongst whom was Betinzoli ». In a note he explaims that the principle as stated by Betinzoli is not to be found in Arconatis' compilation, though it is deducible from some found there.

210. Now Castelli states that he had made experiments, which tended to establish the truth of the principle, and it is possible that experiments made caretessly might confirm hin in the opinion formed, that the velocity increased in the same ratio as the pressure. That the pressure increased as the height was immediately obvious, and it appears a reasonable inference that the velocity would follow the same law. It is therefore quite possible that the increased attention given during the century after Leonardo's death, to the measurement of running water, may have led his successors, independently, to the conclusion he had arrived at; and while there can now be no doubt that to Leonardo da Vinci belongs the houor of having first placed hydraulics on a rational basis as a science, there is room to believe that his successors did not act dishonorably in the matter, but passing through the same stages of observation and deduction, were arrested at the same half-truth.

211. Torricelli, whose scientific mind refused to be contented without a thorough exploration of the matter, carried out some accurate experiments to test the principle. He found on boring holes in the side of a vase, and measuring the quantities of water that flowed out under different pressures, that these quantities, and therefore the velocities, were not as the pressures, but as the square roots of the heights. These experiments have been often repeated since, and with an invariable result. The principle has been found to hold good, and has been generalised into a law, not admitting of any exceptions. The law is the samo as that which rules the fall of solid bodies, as the velocity acquired by these in falling varies as the square root of the distance passed through. The principle has also been found to obtain with the same orifice, when the height of the effluent waters varied. If, for instance, a rectangular aperture is

made in the side of a vessel, and a sufficient amount of water is poured into this to keep the surface at determinate heights above the bottom of the orifice, the quantities of water issuing under different heads are almost proportional to those given by that law; and the differences are accounted for by the action of the sides, of the orifice, and the *vena contracta*.

212. It was not long before the same principle was extended to account for the velocity of water flowing in open channels. The most complete expositions of the principle in its applications, and the theories founded on it, are given in Guglielminis work « Della natura de' Fiumi », in the edition of it annotated by Manfredi, whose remarks are as valuable as the original text. I purpose giving a few extracts from this, as these will be of much use in subsequent parts of this paper. The edition of the work quoted is that of Milan 1821, for which I am indebted to the kindness of the eminent engineer Signor Elia Lombardini.

213. The first chapter is a somewhat abstract discussion of the mechanical principles involved; where it is assumed that water consists of infinitely small incompressible spheres, and results are deduced from various suppositions made respecting them. The second and third chapters are short, the one giving an account of discussions on the origin of rivers, the other the terminology of their parts. It is the fourth chapter in which is found the doctrines embodying Torricelli's principle; and the subsequent chapters may be looked upon as deductions from, or practical applications of the rules there given.

214. The following is an Analysis of Guglielmini's Chapter IV and Manfredi's Notes.

215. A liquid is first compared with a solid, both falling vertically; and Guglielmini shows that while the fall of both is due to gravity only, the solid falls always in the one mass, but the parts of the liquid tend to separate, owing to the different velocities generated in these, and the defect of cohesion between them.

216. In both cases the velocity at any point, when there is no external resistance, is proportional to the square root of the distance passed through from quiescence.

217. When falling through a resisting medium like air, this tends to reduce the velocity of a solid to uniformity, as the resistance generated also increases. In the case of a liquid, the smallness of the cohesion permits the air to penetrate within it, and separate the parts, when it falls in drops.

218. The action of the two is then compared when falling down an inclined plane. He shews, after Galileo, that the motion of a solid follows the same law on the plane, prescinding resistance, that it would in falling vertically; that is, the velocities acquired in different distances are proportional to the square roots, of those distances. Allowing for the effect of resistance, here also the motion tends to become uniform owing to the increase of the friction with the velocity. A liquid flowing in a channel, which may be compared to a plane, follows precisely the same laws, with the following differences in the results.

1.° Difference. — A solid, as a globe, meeting an obstacle in its descent, must, if this is in the line of direction of its centre of gravity, either surmount the obstacle, or be brought to a standstill. Supposing this last to be the case with a globe of ice, which is then suddenly melted, the particles of the water having thus lost the tie that held them together, separate and each follows its own direction.

2.° Difference. — As in falling freely the particles of water most in advance acquire a greater velocity than those behind, the mass tends to attenuate; and when the action of the air comes into play, the water is broken up into drops. In a channel there is the same tendency of the mass to attenuate, and its height of flow to diminish; but owing to only the upper surface of the mass being exposed to the air, this rather holds it all together by its pressure; but between the solid and the liquid « *the second diversity is that the latter attenuates proportionally to the velocity acquired in its fall or descent; while on the contrary solids always keep the same size throughout* ».

3.° Diversity. — The particles of water are disunited and without a strong tie, but not entirely so; and this slight tie acts so that « *one part of the water can not move without drawing the neighbouring part with it; and also, one part can not be retarded, without retarding also that which is immediately contiguous* ». If water were a perfect fluid, that is, without any cohesion of its parts; *and it flowed over a plane or bed, however unequal or rough, the parts that directly encountered the obstacles would be hindered, but not the others* ». But taking into account the cohesion, the parts near the plain would be much retarded, while those more remote would be less so. Whence liquids differ from solids, since « *the impediments of the sloping plane, inasmuch as they retard one part of the solid, retard the whole; but in fluids, a part of the motion is taken away in the nearer parts, but less in the distant ones* ».

4.° Difference. — He then shows by comparing the liquid to a number of small spheres in different layers, that while these tend to move by the common action of gravity, and the layer nearest the bottom is held back by the friction of the plane « *the pressure of the superior layers restores immediately to the inferior all or part of the velocity taken away by the impediment* ». « *If the motion of the fluid is retarded, its surface is raised,* » and « by increasing the pressure of the superior layers, the action of the pressure is proportioned to the degree of velocity ». Whereby liquids differ from solids in raising their surface until « *the ascent equals the descent* ».

219. From these distinctions he deduces eight rules.

Rule I. — « *Water passing from quiescence to motion, whether in issuing from its original springs, or in the melting of the snows, or in any other manner, acquires in its descent through the channels of rivers, which are so many planes, mostly inclined to the horizon, some degree of velocity; but it is quickly reduced to uniform motion* through the great resistance the water encounters in moving ».

Rule II. — « *The course of water being reduced to uniformity, that velocity which it had previously acquired in running down the channel must remain to it, and this is as much greater as is the declivity of the bed* ».

Rule III. — « *The velocity of a river will be greater, when the body of water it carries increases* ».

Rule IV. — « *In rivers, in which the greater efficient height [the height above the low water stage] assists the parts of this impeded, to overcome the force of the obstacles, the less the width of the channel is, so much greater will be the velocity* ».

Rule V. — « *But those rivers, in which the height of the body of water does not increase the velocity, and which continually accelerate, the greater the width, the greater will be the velocity* ».

Rule VI. — He explains that in natural channels there rarely is uniform motion, owing to the changes in the character of the obstacles encountered. Thus « *rivers that run in gravel, notwithstanding considerably inclined channels, are always in a state of continuous acceleration, or retardation; and, on the contrary, those running in sand have a greater uniformity of motion* ».

220. He then draws a comparison between a solid body in the same channel as the liquid, which might be supposed frozen into ice; and shows that owing to the resistance encountered by the solid in the inequalities of the channel and the rigid connection of all the particles, it cannot move; while water can run in the channel owing to its fluidity, the parts retarded not drawing with them equally all the rest. « The fluidity therefore operates greatly in permitting gravity to cause velocity in running water; because, it is through the same cause of fluidity, the water being found in some height of mass, the upper parts press the lower, and with the force of the fall oblige them to receive an impulse to move towards any difference of position, which, reduced to action, produces, in the parts so endued, that precise degree of velocity which the descent from the surface of the water to the place where each is found would give; where-by there is need to confess that *the velocity of the water does not only depend on the descent made through an inclined channel but also on the weight or pressure exercised by the upper parts upon the lower* ».

The above is to be noted as the fundamental doctrine of the italian school.

221. Rule VII. — « Whence it is that *in rivers, near their origin, where they usually have a considerable fall, the velocity of the water results rather from the fall, than from the height of the mass of the water* ». He asserts that this source of the volocity gradually diminishing with the lessenning slope as rivers lengthen out from their origin, « finally all degre of velocity due to the fall is destroyed, » but it becomes due to the efficient height, and therefore « *rivers of small declivity are quicker in their course, as this efficient height of the water is greater* ».

« The course of rivers, then, depends both on the fall and on the height of the water, and a part of the water never receives its velocity but from a single principle; and *the case being given that*, in treating of the whole quantity of water that passes in the same time through a given section, *one part*, for example, the lower, *has the velocity ruled by the efficient height of the water; and the other part*, e. g. the upper, *from the descent*, *some others also are found in which the efficiency of the two causes is equal* ».

He then shews that on this principle, prescinding the resistance of the bed, the velocities of the particles from the surface to the bottom would go on increasing as the square root of the depths; and representing these velocities by parallel horizontal lines, drawn at distances proportional to their depths from a vertical line, the line passing through the other ends would be a parabola, and this would represent the scale of the velocities. Hence his theory is known as the parabolic theory of the flow of water.

222. He goes on to say « This case, however, if it is not impossible, is at least very rare, because the water in more hindered in the bottom than in the surface ». He shows, that the surface velocity may even become the greatest. And he goes on to describe how the necessary impetus to overcome increased obstacles is obtained by an increase of height of the water. He finishes concerning this rule by saying « when *the declivity of the channel* is so small, that the angle formed by the line of the bottom with the horizon is almost insensible, (just as occurs on the italian Reno, where it is only about 32") this declivity *operates little in producing the velocity of the water in the river, except in the parts very near the surface of the water* » and « the parts near the bottom do not run because of the declivity of the channel, but only by the height of the upper water ».

223. Rule VIII. — States that owing to the various resistances of the channel the velocities in the section of a river are not to be found actually following the afore-said rules « from which it appears at first sight to render doubtful any rule for measuring the flowing water ». He, however, proposes one practical method for small channels. He goes ou to say « another cause operates in increasing or diminishing the velocity in parts of the water, from what is due to the fall or the height; and that is the adherence, or viscosity, or colligation, however small, which all the particles of water have together; because as we saw, *the motion being relaxed near the banks, the other more distant parts are similarly hindered, although always less so;* and the thread of the current being joined to that which encounters the bank, its velocity tends to accelerate the neghbouring water, notwithstanding the resistance found there; one thing is out of all doubt, that the lower part being found with sufficient velocity, a portion of this should be communicated to the upper, and that in the same manner the impediments of the bottom retard not only the immediately contiguous water, but also that which is most removed ». « From this it follows that in river floods the waters are rendered swifter; since, by increasing the velocity of the lower parts through the greater height of the water, this

velocity is communicated to the upper parts, through the cohesion of these with those. There is, however, no need to hold any account of these variations in the measurement of the water, since just so much motion as the slowest receives from the communication of the quickest, this much the last loses; and the quickest is not retarded by contiguity of others less quick, without the portion of the velocity lost to the first being gained by the second. Whence it is that by such well adjusted compensation, the sum of the motion neither increases nor diminishes, nor does the mean velocity alter, on which principally depends the measurement of running waters »,

224. He finishes the chapter in summing up « by way of epilogue » « 1) *that the immediate causes of the velocity of the water in rivers are two*, one the declivity of the channel, the other the efficient height of the water. » « 2) *That these two causes do not act together, but only by reason of their prevalence* ». « 3) That in the same section, but not in the same part of the water, both of these causes can have place at the same time ». Other deductions are given, but are not necessary for the exposition of this theory. To complete this, and also to clear up some points, further extracts will be given from Manfredi's annotations.

225. Before doing this, however, the practical results deduced from the theory by Guglielmini's contemporaries, and immediate successors, may be learned from an application of the theory by the Abbate Guido Grandi (given in the Civil Engineers and Architects Journal for 1848, p. 81): « Given two streams, the breadth of the first of which is 700 feet. The velocity of the surface corresponding to the fall of 1 foot (which, according to Guglielmini's table is equivalent to 216 feet 3 inches per minute, that is, $3\ ^3/_5$ feet per second) the [efficient] height of the surface 30 feet, then the whole parabola [representing the scale of velocities] of 31 feet, will be found to be 7175, 88 feet; and subtracting the part 41, 52 feet to which the surface velocity was due, the parabolic trapezium will be 7134, 36 feet, and this will be the scale of the velocity of the section, which multiplied by the breadth gives a quantity of water $= 5,122,113$ C. F. ». Now the greatest discharge ever known of the Mississippi, whose section may be 3000 feet wide by 90 deep, has been shown to be a little over 1,400,000 C. F.; and even then, when accurate measurements were unknown, it was sufficiently obvious that something must have been wrong with the method, or its use, to give such extraordinary figures.

226. Several dissentients appear to have given utterance to objections to the theory; and some of these Manfredi considers in his notes; while he occasionally raises others.

227. In Note VII: Quoting his author who says « it is not to be doubted that the spheres in the superior series falling into the inferior, have in any point of this just the same velocity they would have, if they had come from the beginning of the plane to that point »: he remarks, « Some doubts, in my mind, cannot fail to fall upon this assertion, having regard to the resistance that each globule encounters in its descent, from the contact with those amongst

which it runs, as well as from the bed and the bank ». He goes on to say that this with other reasons might suffice to show a modification in the velocity « and that it thus does not appear evident that each globule in such a descent would conceive all the velocity it would acquire in a free fall. »

228. In Note VIII he says « it is manifest from experience that in fluids the pressure of the upper parts can augment the velocity of the lower ». He shews that it does so in restricting the section and then says. « All the doubt that can remain respecting this is, if the effect of the upper water in increasing the velocity in the lower, can have effect when the section is not restricted ».

229. In Note X he suggests that as none of the positions taken up by his author can be proved by rigorous deduction, it would be better to call his practical deductions hypotheses than rules.

230. Manfredi takes into examination in Note XI the doctrine of the flow of water in channels, with the doubts expressed respecting it. Respecting Guglielmini's opinion he says. « He supposes in the first place, that the water in its first outflow from the vase or receptacle, whence the river draws its origin (since the origins of rivers of any size can always be reduced to this case, although perchance the water is supplied by other streams or smaller rivers) it presents itself there with that velocity which it would have at the same emissary, if no canal were applied there. And in the second place he supposes that in the descent made by the water in the channel, the velocity of each part goes on increasing in the proportion of the square root of the perpendicular descent, made from the beginning of the canal, which beginning he supposes to be the point in which the plane of the bottom prolonged meets the top of the surface of the water in the receptacle; or what is the same, in ratio of the square root of the height measured from the horizontal line of that surface to the part of the water treated of, provided abstraction is made of the resistance opposed to the course of the river. All this is explained in the said place with figure 11; and, consistently with such principles, it follows those he taught in his other work on the measurement of running water, where in book 2 prop 2, he showed that the velocity in any section of an inclined canal is the same it would have if it issued from a vase, through a similar and similarly situated hole of equal section, and as much immersed under the surface of the vase, as is the distance of the section from the horizontal line drawn from the origin of the channel. The same doctrine was commonly followed by the writers that have since him treated of such matters, as Signor Varignon, Signor Ermanno, the Padre Abbate Grandi, Signor di Gravesandi, and others ». This is the most complete statement of the doctrine of the italian school to be found. He goes on to mention and meet the doubts thrown on it, at some length. These will be given as concisely as possible.

231. Some doubts have been moved on the above. The first is, whether the water actually enters the canal with the same velocity when a channel is applied to the apertures, as when there is none; or does the velocity of the water undergo any modification from the bed aud banks of the channel. An

experiment is mentioned where a channel was applied to an orifice in a vase, which was found to cause an increase in the quantity of water. He, however, thinks it desireable to have more extended experiments on this point.

232. The second doubt is not dissimilar; for while it may be allowed that the velocity of the water in the sections may be as the square roots of the descent, it does not follow that this is exactly the same as it would have on issuing freely from a vase.

233. These two doubts he appears to think valid; and he suggests that too much confidence should not be placed in the doctrine as stated; but that by experiment the velocities corresponding to different heights should be learned; as the figures given by the author in his tables are not reliable.

234. Beyond these two difficulties, a third was raised, as to whether in water flowing through channels, the down stream transverse sections which touch those immediately following, do not uphold them, thus taking away the freedom of flow, and do not allow it to acquire the velocity due to the descent or the pressure, a difficulty for which he does not comprehend the foundation. He says « Because were it true that the antecedent section sustained that which follows, it appears to me that such a support could occasion no diminution of the velocity that this would have acquired from the cause acting upon it, but would have no other than the effect of an impediment keeping the plane of water passing in an instant through that section, from changing its figure (which we may suppose rectangular), and flattening out on the bottom of the canal, as it would do if not sustained, but necessarily keeping it upright, and each part of it going with the velocity with which it is affected, without any point diminishing; and the reason is, because no one body preceding another can cause any obstacle to the hinder and contiguous one, when the first flies with a velocity equal or greater than that with which the second advances ».

235. A doubt was also thrown on the statement that the upper layers pressing on the lower ones generate in these an increase of velocity. He says « Postulating then this doctrine, as a hypothesis, at least, it remains to note well, that while the upper parts of a section, although running, have power to impress on the inferior that degree of velocity which agrees to their height and pressure, in the mode said, it does not however always produce entirely or in part that effect ».

236. These selections and extracts give the means of estimating fairly the parabolic theory as conceived and used up to the middle of the eighteenth century. Doubts still remained over it; and when Couplet experimented on the flow of water in conduits at Versailles in 1732, the Academy of Science thus expressed themselves (1).

« He (Couplet) comes now to the most difficult point of all this matter, to the diminution which physical accidents cause in the expense of the water

(1) DARCY, *Recherches expérimentales.*

snch as friction causes against the interior walls of the conduits, the sinuosities of the conduits, and the air found intercepted there ».

« The rule that the velocities of the water are as the square roots of the height through which it has fallen, or, what is the same, of the height of the column of water whose charge makes the lower water flow, is extremely deceptive in the large conduits such as those of Versailles, which are sometimes 2,000 toises in length. If one judged hy this rule the quantity of water which ought to flow it would be 407 inches in place of the 10 $^1/_2$ which really came in the experiment made by M. Couplet ».

237. It is evident there was some thing very wrong with the rule, or with the mode of considering it; and the former view was taken hy M. Duhuat; who, in 1779, first gave his views, founded on a discussion of the experiments of the Abbé Bossut. He further undertook experimeots on a more extensive scale, confirming himself in the new principle he had formed, and he issued these in 1783 under the title of the Principles of Hydraulics, a work he subsequently extended, and published in two editions, the last in 1816. From this will be taken a few selections so as to present his theory, where it differs from those of his predecessors, in his own language.

238. The preliminary Discourse, p. XI says: « When it had been discovered, by experience, that the velocity of water issuiog from a vase through an orifice, is proportional to the square root of the charge, attention was given at first to verifying this surprising enough fact; aud by a strange mistake, it was wished to apply this principle to every kind of movement of water. Varignon, Mariotte, Guglielmini, and all the rest, made it the base of hydraulics. Tbis last calculated by the parahola the velocities of the filets at different depths. and supposed it nothing at the surface, greater helow, and greatest at the hottom. It was after this hypothesis that he assigned the expense of the Danube in his measnrement of running waters; he was meanwhile too good an observer, as well as Mariotte, not to perceive that, in conduit pipes, and in beds of rivers, the frictioo of the water against the walls alters and denatures entirely the order of the velocities from an orifice.

« Guglielmini meantime rectified afterwards his ideas, after a great number of observations which he made on the courses of rivers » « he thonght that the water in augmenting the depth, acquired by pressure the force of the fall lost ».

239. He proceeds p, XX to discuss the pratical state of the science and says « No one will deny that if two rivers have the same depth, the same width, and the same slope, and run on a homogenous bed, their velocities will not differ; hut if any ooe of these accidents changes, their velocity will increase or diminish, without ceasing, nevertheless, to be uniform. Up to the present no theory teaches how to calculate the velocity, from these data; oow, the velocity being unknown, the expense is so also; and hy a necessary sequeoce, one can not foresee the success of any operation on the bed of rivers, nor resolve a single problem relating to it ». He further says, p. XVI — and

hore is the difference between his theory and that of Gugliemini — « if water were perfectly fluid, and it ran in a plain infinitely polished, from the parts of which it would not suffer any resistance, it would accelerate its movement in the manner of bodies which slide upon inclined planes: for it is evident that the slope of the surface is the sole efficacious cause that engenders movement, since without it the movement has no place ».

240. The contrast of the two principles shows that Dubuat holds that water in passing down a channel tends to move exactly as if it were a solid; and when no resistance has place, the lower strata move with exactly the same velocity as the upper, the motion being due to their wsight acting down the slope of the channel.

241. Guglielmini on the contrary asserts that in addition to the motion generated as Dubuat admits, there is an additional velocity, or rather, as he expresses it, an alternative velocity, generated by the pressure of the upper layers on the subjacent ones. These are the primary differences; but it is necessary to let Dubnat speak further.

242. Chapter II, para 14: « We propose to examine, in the course of this first part, the laws that water, in virtue of its fluidity, follows, in order to run in the beds of rivers, in canals and in pipes, in considering above all the velocities it acquires there, by reason of the slope and the dimensions of the bed that holds it. This matter has been so slightly considered up to the present, despite the ntility that an exact theory on this part of Hydraulics should have, that it may pass for new. At most the commonest notions upon the course of the water have been barely touched on; and all kinds of very serious faults are daily committed in this way, as a natural sequence of the ignorance in which are these who manage the waters, of the principles of a science in which the welfare of society is so much interested. Meanwhile our errors are, in this matter, of a different conseqnence from those in objects of taste, of lnxury or of comfort; sipce thus always results, either a real damage, or the loss of some precious advantage.

243. « The movement of the water that runs in the bed of a river, to throw itself into the sea, is due to two causes, 1.° to the action of its weight, which compels bodies to descend incessantly, when no obstacle arrests them; 2.° to the mobility of the particles of water, which makes them take a perfect level in closed vessels, or determines them to flow to the side where they find the least resistance. In fact, if an element of the water, a primitive molecule, is conceived as pressed on all sides by the other neigbouring elements that surround it in a firm vessel, the pressure it suffers being equal from all parts, it should remain in repose; but if the surface of the water is not level, and it lowers on one side, as happens in the current of a river, the differences of height found between the column of water pushing that element from behind, and that which tends to push it from before, is the force which compels the element to move to the side where the pressure is least: and it obeys this effort with much facility, because of the extreme mobility of the parts of the

water. It is seen by this that all the elements situated at different depths, are pushed with an equal force in the direction of the current, and that this ought consequently to tend to move with equal velocities, from the surface to the bottom ».

244. This last paragraph contains the foundation of Dubuat's principle; and it may be contrasted with Guglielmini and Manfredi's statement of the same subject. He says the moving forces being due to the difference of pressure, and these being equal at all the depths, the velocities at all depths tend also to be equal. They say the pressure on each particle being due to the depth, and by Torricellis principle the velocities being as the square roots of the depths, the velocities tend to increase in that ratio from the surface to the bottom. The difference is an essential one, aud merits full consideration.

245. Manfredi's arguments that the pressure in front does not hold the particles back, owing to all the particles at the same depth moving with the same velocity appears a sufficient answer to Dubuat's assertion in this case.

246. Dubuat's principle, however, was immediately on pubblication received as the true one, and at once superseded its predecessors. It appears to have been accepted without hesitation or dissent, and for nearly a hundred years it has passed almost unchallenged, and is the foundation on which all the hydraulic works of the century have been based. So entirely has it occupied this position that while it is generally repeated in all text books, in one — an italian one, — the Treatise on Hydrometry of Professor Domenico Turazza, it is claimed for Torricelli himself in these words » Torricelli, for the first time, in his writing on the Val di Chiana, states concisely this fundamental hydraulic truth when he says « that the velocity of water does not increase or diminish according to the slope of the bottom but is conformed to the increased or diminished slope of its upper surface ». Lombardini is the only writer I have found to hint at a possibility of the old theory being the true one, and his expressions are carefully guarded, and may be read without being so interpreted.

247. It is, nevertheless, with the express purpose of showing that there is reason to accept Torricelli's principle as the true one to explain the flow of water in channels, as well as from orifices; and with the view of deducing a formula from that and other acknowledged principles of mechanics that this paper has been undertaken.

248. Before proceeding to this, a brief review of the different formulae which have been founded on, or derived from Dubuats principle will be of advantage. These are numerous, and various. But as several follow the same type, differing only in the numerical coefficients, only the type of the class need be considered; and the different types compared. Fortunately, this task has been already accomplished by M. Bazin in the Annales des Ponts et Chaussées for January 1871; and this will be followed. As the task undertaken of attacking a theory apparently established, and acquiesced in for a century by all the distinguished hydraulicians who have written, is an exceedingly difficult and delicate one, the rule I have hithertoo followed will be persisted in, of making

any writers of whose aid I can avail myself, express in their own terms the opinions or views which, whether for or against, will serve for the elucidation of the subject.

249. The occasion that gave rise to M. Bazins paper was the following: — In the first half of the last decade two extremely important hydraulic works were written and published without the writers of each knowing anything of the others. M. Bazin, who continued the researches of M. Darcy, presented to the Academy of Sciences in 1863 his Hydraulic Researches, containing extended experiments on the flow of water in open channels; with formulae deduced from these. The work was not published till 1865. In 1861, an elaborate report on the Mississippi with an investigation of the flow of water, and new formulae by Messrs. Humphreys and Abbot, of the U. S. Engineers, was presented to the United States Government. It was afterwards published. A great diversity was apparent between the two sets of formnlae; and Major Abbot in 1868 read a paper comparing them, and endeavouring to show that the American formulae gave better results than those of M. M. Darcy and Bazin. Other writers in Europe entered on the question, some dissenting from both sets of formulae, aud offering new ones of their own. M. Bazin, dating his paper the 5th July 1869 replies, and further gives a discussion of the different types hitherto published.

250. These dating from the time of Prony and Dubuat are of two classes:

« 1.° Monomial formulae derived from that of Chezy by the introduction of fractionary exponents.

2.° Binomial formulae in which the coefficient A is expressed by a function of two terms of the three variables R, U and I ».

$$I = \text{the sine of the inclination}$$

$$U = \text{the mean velocity of a section}$$

$$R = \frac{\Omega}{\chi}$$

where

$$\Omega = \text{the area of the section}$$

$$\chi = \text{the perimoter of do}$$

and

$$A = \frac{R\,I}{U^2}$$

251. « The monomial formula can always be expressed by

$$R^m\,I^m = b\,U^r$$

or:

$$A = \frac{b\,I^\alpha}{R^\beta\,U^\gamma}$$

α, β, and γ being always less than unity ».

252. « M. de S. Venant proposed » in 1851 the well known formula

$$R\,I = 0{,}000401\,U^{\frac{21}{11}}$$

founded on the old experiments, aud on the new ones becoming known, he generalised it into

$$A = \frac{b}{U^{\beta}\,R^{\gamma}}$$

where b, β and γ vary with the rugosity of the walls.

253. « M. Gauckler has, on his side, deduced from our (M. M. Darcy et Bazin's) experiments a double formula which he presents thus.

1.° Canals whose slope exceeds 0,0007

$$\sqrt{U} = a\,\sqrt[3]{R}\,\sqrt[4]{I}$$

2.° Canals whose slope is below 0,0007

$$\sqrt[4]{U} = a\,\sqrt[3]{R}\,\sqrt[4]{I}$$

254. « In raising these to the 4th power and designating by b the coefficient $\frac{1}{a^4}$, these two expressions become

$$A = \frac{b}{\sqrt[3]{R}} \qquad (2)$$

and

$$\text{and } \frac{R\,I}{U} = \frac{b}{\sqrt[3]{R}}$$

comparing these with M. de S. Venant's, M. Bazin says: « The two formulae (1) and (2) can be expressed by

$$A = b\,x^{y} \qquad (3)$$

255. He then gives a graphic representation of the results of these formulae compared with the experiments and shows that the « monomial formulæ are little suited to the general representation of the experiments upon earthen channels ».

M. Gaukcler having recognised these proposed a second for channels in earth

$$\frac{R\,I}{U} = \frac{b}{\sqrt[3]{R}} \qquad (6)$$

256. M. Bazin criticises this and shews that except in very narrow limits it is inapplicable. He goes on, « M. Bornemann in criticising the double formula of M. Gauckler proposes the unique espression to replace it

$$\sqrt[4]{u} = a \sqrt[3]{R} \sqrt[5]{I} \tag{7}$$

« This differs very little from formula (6), and raises the same objection; the introduction of the factor $\sqrt[5]{I}$ in the second member ameliorates the results very little. We will therefore not dwell upon it more. »

257. Mess. Humphrey and Abbots formula as originally given was

$$b = \left(\left[222\, r,\, s^{\frac{1}{2}}\right]^{\frac{1}{4}} - \dot{0},0388\right)^2$$

Their later was more complicated. Denoting with a the area, p the wetted perimeter, l the width, s the total slope, divided into two parts i and h, the second being due to changes of section and bends, Professor F. Brioschi gives the formula reduced to metric measures as

$$V = \left[\sqrt{(m + \sqrt{68,72\, \rho}\, \sqrt{i})} - \sqrt{m}\right]^2$$

where

$$m = 0,0025 \frac{0,933}{\sqrt{r} + 0,457}\, r = \frac{a}{p,}\, \rho = \frac{a}{p} + l$$

a formula unrivalled in complicacy.

M. Bazin continues:

« When the complicated formula given by Mess. Humphrey and Abbot is simplified by the suppression of the two least important terms » « it is reduced to

$$U = a \sqrt{R} \sqrt[4]{I}$$

which elevated to the square is

$$A = b \sqrt{I} »$$

After criticising it he says « it is then inadmissable for artificial channels of strong slopes; we will see further on how it suits the natural channels in view of which it has been specially establisbed. »

258. Proceeding then with the enumeration he gives from the memoirs of the Berlin Academy 1865, M. Hagens formula

$$U = 2,425 \sqrt{R} \sqrt[6]{I}$$

or

$$A = 6 \sqrt[3]{I^2} \tag{9}$$

this resembles the above, and does not call for special remark ».

Binomial Formulae.

259. « Old formula of Prony and Eytelwein, expressing A in function of U ».
« This formula, which has been in use for a long time is

$$R\,I = a\,U + b\,U^2$$

or in dividing by U^2

$$A = a + \frac{b}{U} \tag{10}$$

« It is no longer admissable as a general formula » when the section of the channel is very small, the constant term a can be neglected before the variable term and it is

$$A = \frac{b}{U} \quad \text{or} \quad R\,I = b\,U \tag{11}$$

In both cases the coefficients are variable with the slope and the nature of the walls.

260. « Formula of M. M. Darcy and Bazin, expressing A in function of R ».
« We have extended to open channels the formula

$$R\,I = \left(a + \frac{\beta}{R}\right) U^2 \ \text{or}\ A = \left(a + \frac{\beta}{R}\right) » \tag{12}$$

For channels of very small slope it has been found necessary to modify it to

$$A = a + \frac{\beta}{R\sqrt{I}} \tag{13}$$

This was afterwards renounced on later experiments being made in 1866.

561. He then discusses the complicated formula of Humphreys and Abbot given above, which he reduces to

$$\sqrt{A} = 0{,}167\ \sqrt[4]{I}\left[1 + \frac{0{,}096}{\sqrt[4]{(R + 0{,}457)}}\,U^2\right] \tag{15}$$

He sums up the discussion by saying « Whatever the theoretical considerations may be, we believe we have shown that the american formula is in

complete contradiction with the *ensemble* of all the european experiments, and that, in every other case than that of great rivers with feeble slopes, its use may lead to very considerable errors. »

262. Amongst the many writers in Europe who entered into the discussion of the formula was M. Kutter, a swiss, who in 1868, published a paper where he took into examination the suitability of the theory of Humphreys and Abbott to the mountain streams, and channels of great slope. An account of this and other papers is given by Signor Bosco, read before the College of Engineers of Milan (1). Comparing the formula of Humphreys and Abbott with those of Darcy and Bazin, M. Kutter arrives at the following conclusion:

« That the formula of Humphreys and Abbott offers exact results for currents with gentle slopes, while it does not agree with currents of strong slopes.

263. In a further paper in 1869 he gave a large selection of experiments, divided into four groups, and compared them with the results given by five formulae; those of Eytelwein, Humphrey and Abbott, Bazin, Gauckler and a new one of his own. His results are with respect to the first two groups of data, containing river experiments only.

« 1.° That the formulae of Gauckler and Eytelwein give unsatisfactory results.

2.° That the formula of Humphreys and Abbott gives good results.

3.° That the formulae of Bazin and Kutter give unsatisfactory results respecting the Mississippi, and respecting the others, results not always good ».

274. With respect to the last two groups of experiments made on smaller streams he says:

1.° That the formulae of Eytelwein and Humphrey do not entirely satisfy the results of direct measurement.

2.° That the others agree with them sufficiently well, and that the formula of Ganguillet and Kutter offers the least difference amongst the results of the experiments. »

265. M. Bazin, in his paper, gives the formula of M. M. Ganguillet and Kutter, who take as point of departure

$$A = a\left(1 + \frac{K}{R}\right) \qquad (16)$$

which they modify in replacing A and R by $\sqrt{A}$ and $\sqrt{R}$ whence

$$\sqrt{A} = a\left(\frac{K}{\sqrt{R}}\right)$$

where $\frac{1}{a} = a + \frac{l}{n} + \frac{m}{I}$, $K = \frac{n}{a}\,l$, these constants being $a = 23$, $l = 1{,}00$,

(1) *Atti del Collegio degli Ingegneri ed Architetti in Milano*, vol. IV, fasc. II, 1871.

$m = 0,00155$ and n a coefficient of the rugosity of the wall. This formula then becomes

$$U = \frac{23 + \frac{0,00155}{I} + \frac{l}{n}}{1 + \left(23 + \frac{0,00155}{I}\right)\frac{n}{\sqrt{R}}} \tag{17}$$

« A formula sufficiently complicated » It can be written

$$\left(\sqrt{A} - \frac{n}{l}\right) = K\,a\left(\frac{I}{\sqrt{R}} - \frac{I}{l}\right) \tag{18}$$

comparing these with his own formula he shows that it does not represent the results of the larger rivers at all.

266. M. Bazin requires different sets of coefficients to his formula for every variation in shape or degree of rugosity of the channels; but in his larger work, he takes an average of four kinds of rugosity, and by restricting the use of the formula to rectangular or trapezoidal channels, four sets of coefficients are obtained which give approximately correct results for small channels.

M. Gauckler gives no less than twelve sets of coefficients for as many kinds of channels for his own formula. M. De Prony in his work (Recherches Physico-Mathematiques, t. 1804) p. 50-53, gives an account of the progress of the formula from the monomial to the binomial stage, where this depends on more than one term containing powers of the velocity.

267. Dubuat's formula with the metre unit is:

$$U = \left[\frac{\sqrt{243,79}}{I^{\frac{1}{2}} - \frac{1}{2}\log.(I + 1,6)} - 0,049359\right][I - 0,016453]$$

268. Chezy who had been engaged on the subject eleven years before the publication of the second edition of Dubuat, proposed

$$\frac{R\,I}{U^2} = a \text{ or } A\,I = a\,\chi\,U^2$$

(calling A the area), which is the type of the monomial class.

269. Gerard founded his formula on the researches of Coulomb, using a term with the first powers of the velocity to express one portion of the resistance, and an other with the second power for the rest.

His formula is

$$R\,I = a\,(U + U^2)$$

or:

$$A\,I = a\,\chi\,(U + U^2)$$

270. De Pronys' own formula was:

$$A\,I = \chi\,(C + a\,U + b\,U^2)$$

but as C was relatively small and unimportant this became:

$$A\,I = \chi\,(a\,U + b\,U^2)$$

which is the type of the binomial dependent on different powers of the velocity.

271. Darcy and Bazins formula

$$A\,I = \chi\left(\alpha + \frac{\beta}{R}\right)V^2$$

(A representing here the area) is a binomial, but not of V. It is the type of a separate class, and it forms a most important departure from the former type. Its authors do not attempt to give it anything but an empirical basis; and hardly realise its full importance, for by pushing the divergences a little further, and substituting $\frac{1}{R^2}$ for $\frac{1}{R}$, with other seqnences, most important empirical results are obtained, and a theoretical solution of the problem is suggested.

272. Other writers than those mentioned above-the late Rev. Canon Masely in England and Professor Colding in Denmark-have within the last year or two discussed parts of the subject; bnt have not tonched on the particular branches relating to the flow of water in channels, in terms of the section and slope of the channels.

273. The earlier writers, as Dnbuat and Prony attempted to deduce their formulae from theoretical principles; but the later ones have branched out into two well defined schools, the one theoretical, the other practical. To the last belong nearly all the writers whose formulae are given above, and it appears a cardinal principlo of the school to reject all pretension to a deduction from theory. Indeed, it would be quite impossible to justify some of the formulae on theoretical grounds; and they differ so widely from each other that if any one were right the rest must all be theoretically wrong. Theoretical discussions may be given, but in determining the degree in which the different terms enter, only empirical conditions are attended to.

274. An instance of what is meant may be taken from Humphrey and Abbott's work, where after a theoretical discussion in which they give the formula

$$C = \frac{A\rho}{(p + w)\,z^2}$$

they proceed:

« The several members containing only known terms its numerical value was computed for the different observations already described, and it was at

once evident that c could not be assumed constant. To detect its law of variation the different values were plotted as ordinates to the corresponding values of $\frac{a}{p + W}$, V, and S, successively, as abscissae. While serrated curves, following no apparent law, resulted when C was plotted with $\frac{a}{p + W}$ or V, a quite uniform result was obtained by using S. It was then reasonable to conclude that C was some function of this quantity. Much labor was expended before an equation representing this function was found. At last only the data obtained on the regular field work of the Survey were used; then in succession the data discussed above for the higher slopes were added. The successive additions modified the results already obtained, by requiring a change for the higher slopes. To give a detailed account of these trials would extend the discussion beyond its proper limits without answering any useful purpose. Suffice it to say that few classes of continuous curves for which equations of conditions for passing through two, three or even four points can be conveniently computed, were left untried. There seemed to be some fatality from which there resulted either large descrepancies for some of the observations; or an absurd result when the quantity S [the slope] approached its maximum real value, unity; or an expression so complex that it approached an equation of the third degree or higher, when solved with respect to S; or the necessity of leaving the curve and following a tangent, for slopes above a certain limit. At length it was discovered that a very simple curve

$$C = \frac{S^{\frac{1}{2}}}{195}$$

would fulfil certain necessary conditions, which could not be forced upon curves whose equations are of a mnch higher degree. It was accordingly adopted. »

275. An extract from M. Darcys book (Recherches esperimentales etc.) will also bear on this point; speaking of his formula

$$\varepsilon \left(R \frac{d v}{d r} \right)^2 = \frac{r i}{2}$$

where R is the radius of the section, and it appears in a different manner from that given by other authors, he says: « To arrive at a rational appreciation of the intervention of the absolute size of the section in the expression of tho resistance due to the interior actions of fluids, it would need to know the mode in which this intervention has place; it has not been permitted to me to arrive at the philosophical interpretation of this law, which experience alone has revealed to me, for pipes and even for rectangular channels, as I will show in a work which is at present the object of my studies. »

276. It is in a large measure due to this careful and patient labor, combined with the great ability of those engaged in the investigation, and to the total

departure from the theory of Dubuat, which served as the starting point, that the results of experience have so well been represented by the various formulae. And it is also because these same formulae have not a theoretical basis, that they fail when extended beyond the limits within which they have been derived.

277. The writers of the theoretical school have failed entirely in their attempts to found a formula on the received theory, and practical writers seldom refer to them except to state their dissent from the conclusions arrived at. And their divergence from each other is not less wide than that of the practical formulae already given. The principal theoretical writers are M. Navier, M. Sonnett and M. Dupuit.

278. In M. Bazin's work (Recherches etc. pag. 28) an extract is given from M. Sonnett's work, where the latter says: « M. Navier is, to my knowledge, the sole geometer who has occupied himself with the problem of flowing water in all its generality...... Unhappily he has deduced from his calculations but a small number of applicable results ».

279. M. Dupuit says of M. Navier's labours (Etudes etc. pag. 14). Thus all the calculations of M. Navier, who otherwise keeps himself to theoretical generalities, end only in results that are completely erroneous if applied to practical cases. There only ought to remain from this memoir the expression $\varepsilon \frac{dv}{dz}$, of the resistance due to cohesion, which is an advance on the inexact notions presented by M. Prony ».

280. M. Bazin says of M. Sonnett and M. Dupuit's (Recherches etc. pag. 28) works: The formulae to which M. Sonnett has arrived agree no better with the results of experience. According to them, in effect, the difference between the velocity at the surface and the velocity at the wall would be proportional to $R^2 I$, while all the experimental data agree in indicating that this difference and the velocity themselves are proportional to $\sqrt{RI}$. In a work remarkable in many respects, M. Dupuit, departing from the hypotheses of M. Navier, has arrived at formulae analogous to the preceding, and consequently raising the same objections ».

281. General Morin, as Secretary to the Commission of the Academy of Science, reporting on M. Bazin's works, says; « Many geometers have sought in these later times by the aid of hypotheses, more or less ingenious, to submit these delicate questions to calculation; but, as their hypotheses were not completely conformed to the true circumstances of the movement of liquids, the consequences to which they have been conducted are not found to accord with observation, even in the case of uniform movement ».

« The solution of this question has then, in common with so many others in physico-mathemetics, evaded the mathematical analysis. The Engineer, who in the mean time, has need of rules to guide himself in application, is thus forced to recur to observation, and to control himself with empirical formulae which will represent these results ».

282. It is not necessary to add anything to these extracts, which fairly describe the present position of that part of the science of hydraulics, which is connected with Dubuat's theory. We may reasonably conclude from them, that that theory has entirely failed to justify its occupation of the place it at present holds as the only one that can adequately account for the phenomena of flowing water. A theory based on Torricelli's principle has in its favor the analogy that may be presumed to exist between water flowing in a channel, and flowing out of orifice, and if, as an hypothesis only, this analogy be assumed, and it can be shewn that a formula deduced from that principle answers where all the others fail, there will be reasonable grounds to revert to that principle as the true one.

283. Guglielmini's theory with a few modifications will serve as a starting point.

1.° Instead of saying with him that while the velocity of the water flowing in an open channel is due to both the surface fall and the depth of the stream, only one of these causes can be in operation on each particle at any time, it will be assumed that the acceleration of each particle of water is due both to the surface fall and to its depth below the surface, and that in no case can one of these causes come into operation without the other; the actual acceleration being the effect of both.

2.° Instead of assuming that the velocity due to the depth is *the same* as it would be were the particle issuing from an orifice in the side of a vessel, it will be assumed that the acceleration is *proportional* to that velocity; that is, that it is proportional to the square root of the depth below the surface.

On this assumption, the sum total of the acceleration due to the depth, on a unit of width, will be proportional to the depth of the centre of pressure P below the surface:

$$P = \frac{\int x^2\, y\, d\, x}{\int x\, y\, d\, x}$$

And the mean velocity will be proportional to $P^{\frac{1}{2}}$

3.° The declination of the surface not only contributes its own share to the velocity, but it determines the proportion the actual velocity of acceleration holds to the velocity that would be acquired from the depth were the water flowing freely from an orifice.

That is if D be the declination, $D^{\frac{1}{2}}$ will be the velocity due to the surface slope; and $P^{\frac{1}{2}}\ D^{\frac{1}{2}}$ will represent the velocity of acceleration due to the two causes acting together on a mass of water flowing in an open channel.

We thus have the velocity of acceleration, on a unit of width, given by Torricelli's principle as $P^{\frac{1}{2}}\ D^{\frac{1}{2}}$, instead of $D^{\frac{1}{2}}$ on Dubuat's, as accepted by all the writers whose formulae have been referred to.

284. Another modification, unimportant in its consequences to the formula, may be made. Some physico-mathematicians, as the late Canon Moseley, assuming

water to be almost a perfect fluid, shew that the different layers of it gliding over each other transmit the velocity from the quicker to the slower ones by mere impact. Other writers, as Guglielmini, suppose it to be transmitted by the viscosity of the water alone. The question is a purely physical one; and so far as mechanics are concerned, whether one of the two acts alone, or both combine to produce the result of the transference of force through the water, is immaterial, and in assuming that both concur we can use the reasoning of both classes of writers without further remark.

285. An experiment to contrast the result of the two principles will now be described. Dubuat in his (Part. II, chapter I, p. p. 81–83) work, shews that according to his principle the discharge of an open channel is independent of the shape, and depends only on the mean radius; that is, the area divided by the perimeter. In the case of two or more channels with the same area, and tho same perimeters and slopes, but of different shapes, the velocities of the water would be equal. M. Bazin devotes a chapter of his large work to this subject, where he shows he made several experiments on the flow of water in channels of different shapes, and his conclusion is: The figure (Recherches Hdraulyque, p. 98) of the transversal section appears then to exercise no influence great enough to take into account in the application when this figure is a rectangle, a trapezium or a triangle. The comparative experiments which have conducted us to this result have only been made, it is true, on boarded walls; it nevertheless appears probable that more resisting walls lead to analogous conclusions ».

286. I caused four small channels to be prepared each with an area of one square inch, and three inches perimeter; and all ten (10) feet long. N.° 1 was a square; N. 2 a rectangle half an inch deep; N.° 3 a triangle 2, 5615 inches wide and 0, 78075 inches deep; N.° 4 a triangle 1,5615 inches wide and 1, 28075 inches deep. They were all securely fixed in a frame, and screwed to straight edged pieces of wood, held perfectly straight with their upper edges transversely level. A fall of $^3/_{10}$ of an inch was given in ten feet. The upper ends were fixed in the side of a box 2' 6" $\times$ 1' 6" $\times$ 9". I had some difficulty in giving a regular supply of water, so that each in turn should flow with the water level with its upper edge. At length I devised a balance module (a specification of which is attached) and succeeded fairly in giving a regular flow from large half casks with varying depths of water. All the channels were of teakwood, carefully made, and in the same condition as to smoothness.

287. The water was received from each channel in the same vessel, and the times noted. It was found that as the experiments were more carefully made, the pipes kept more perfectly straight, and their upper surfaces brought more accurately to the same level transversely, the experiments approached nearer and nearer to one result.

288. That result on the last occasion tried was.

Pipe	N.° 1	N.° 2	N.° 3	N. 4
Time in Seconds	68, 6	96, 16	91, 3	83, 3

The velocity would be inversely as the times, and taking the velocity of the first as 100, would be as.

N.° 1	N.° 2	N.° 3	N.° 4
100	71,2	75,1	82,3

Now on Torricelli's principle with the modification of Guglielmini's theory above proposed the velocities would be.

N.° 1	N.° 2	N.° 3	N.° 4
100	70,7	70,5	89,3

But on Dubuat's principle they wonld all be equal, since the mean radius is the same in each,

289. It may be objected to the experiment that it was done on too small a scale; or that some strange error has crept in to make the result agree with a foregone conclusion. It is true the result was foretold long beforehand, as it was believed, on theoretical principles alone, to be the only possible result; but on the occasion referred to, when the above figures were obtained, the conduct of the experiment was placed in the hands of two competent Assistants, niether of whom were interested in the theories, one watching that the level of the water about the centre of the length of the pipes was even with the sides, the other regulating the ont flow at the module. I took the time. It is, however, still possible that owing to the smallness of the scale, or some error that has escaped notice, these results have been brought about; and nntil the experiment has been repeated, and disproved or verified, all that will be asked for it is, that it may be accepted as an illustration of how the results of the two principles diverge, and wherein they disagree.

290. We have now to show, on theoretical grounds, reasons for accepting Torricelli's principle as the true basis of a formula. To do this another well known principle, that of living force, or *vis viva*, will be used. It will be convenient to call it the principle of Leibnitz, who first originated the term. This is the same principle as that on which some of the preceding formulae profess to be founded; and its use is attended with difficulties.

291. Before applying it, a principle will be formally postulated. This principle is the foundation of all scientific reasoning; and corresponds to the Principle of Excluded Middle in formal logic. It may be called simply the Principle of Exclusion. It postulates that when a function adequately fulfils certain conditions, no function, not identical with it, can also do so. The only objection that can be made to this is, that it is adready universally acknowledged, and it is superfluous to enunciate it. Those who think so will hardly be prepared for the deduction to be made from it.

292. De Prony (Recherches Physico-Mathématiques, p. V) says: « The results of the observations consigned in the *Principles d'hydraulique,* and the classification their author has made, with much sagacity, of the different kinds of re-

sistances manifested in the movement of fluids might have conducted him to express the sum of these resistances by a rational function of the velocity, composed of two or three terms only; but the glory of the discovery was reserved for M. Coulomb »... who « proves by reasoning and by fact, that, in the movements he has observed, the phenomena are satisfied in equating the resistance to an entire and rational function of the velocity, composed of two terms only, one of which is proportional to the first and the other to the second power of this velocity ».

293. Newton says something like this in his Principia (Book II Prop XIV Scholium) « The resistance of spherical bodies in fluids arises partly from the tenacity, partly from the friction, and partly from the density of the medium. And that part of the resistance, which arises from the density of the fluid, we may call in the duplicate ratio of the velocity; the other part, which arises from the tenacity of the fluid, is uniform, or as the time ».

294. Now in every conceivable case in mechanics where the amount of living force in a body can be measured, the principle of Leibnitz adequately represents it. This principle affirms that the living force of a particle, or more generally, of a body all the particles of vhich have the same velocity, is the product of its mass into the square of its velocity. A globe moving through a resisting medium has the living force lost measured only by taking its velocities at different times, and the difference of the squares of these multiplied by its mass represent the loss.

295. So a body of water passing through a section may be conceived a system of points each with its own velocity, the living force gained or lost being represented by the sum total of the particles each multiplied by the square of its velocity, and the differences of these sums at different times give the amount of force gained or lost. And in every conceivable case this is true. The force can only be represented by the mass into the square of the velocity.

296. This representation of force is undoubtedly one of the most difficult parts of mechanics, and when the distinction was first made of living force and inert force, it seemed natural to extend the measurement of the first to the latter. But some later writers have thought it better to leave out the term inert force as conveying no exact idea. In fact, the only way in which the force of a resistance can be expressed, is in terms of the living force consumed in overcoming it. This holds true both of the resistance of armour plate to the impact of a shot, and of the resistance of the air in the interval between the shot leaving the gun and its arrival at the plate. Both resistances, however the mind may conceive them as operating, can be expressed in terms of the mass of the shot, and of the square of the velocity at different times. It follows then that if the second power of the velocity does adequately in conjunction with the mass represent the force, no other power of the velocity can do so, nor can any combination of the second power with others do so.

297. The principle of exclusion applied to the hydraulic formulae sweeps out at once all those of Prony's type founded on more than one power of the velocity.

298. If this principle of Leibnitz had been rigorously adhered to in its intetegrity, viz: that whenever a force whether accelerating or retarding is shewn, the mass of the matter, with the square of the velocity must also be shewn, Dubuats theory would never have heen received as applicahle to the flow of water in motion.

299. In reviewing the expression for the accelerating and retarding forces equated to represent the relation of the terms, De Prony says « After this manner of viewing the accelerating and retarding forces that act on a fluid mass, the equations of the particular movements of each of its elements can be placed out; and the immediate examination of these equations, which are hereafter found, gives place to the suspicion of some *lacune* in the series of facts by which the uniformity of movement recognised is explained ».

What this lacune is will appear when the accelerating and retarding forces are fully stated ont on Leihnitz' principle; and it will appear much more serious than Prony could have imagined.

300. M. Darcy conceives Prony to allude to the curious enough fact that the equations referred to show the resistance as a function of the velocity at the wall, and not of the mean velocity. As Prony had just noticed that fact without connecting it with his suspicion of a lacune M. Darcy's opinion is hardly sustained.

301. We may conceive a mass of water flowing in a ohannel whose area is A to have arrived at uniform motion, or at least to be oscillating continually in a very small range ahont uniformity, when the mean velocity will be V. If during one of these oscillations a second of time be taken, the middle of the time being exactly when the velocity arrives precisely at V, we can conceive for that second the retarding forces to be suspended, and the accelerating forces to be in operation. The mass of matter passing through the section during that second will be almost equal to AV; and the accelerating force generated in the mass dnring that second will be represented on Dubuats principle by A V D, and on Torricellis by A V D P. The term for gravity may be here neglected conformable to general usage.

302. There is here the former distinction kept up between the expressions on the two theories. Strangely enough none of the formulae either of Chezy's or Prony's type represent the accelerating force hy A V D, as they only give usually A D.

303. An exception appears in Messrs. Humphreys and Ahbott's work, where they represent the accelerating force as in action over a length of the channel l. Their formula is on that side $G. g\, a\, l \frac{h}{l}$ when G and g represent the density of water and local force of gravity, and may be left out. The other factors are $A = a$ the area, l the length, $\frac{h}{l} = I$ the sine of inclination. This raises a very important qnestion, what length of channel can he introduced in a formula expressing uniform motion. Suppose the velocity V is 5 feet, and l is taken at 1000 feet, in the

whole of whicb acceleration is going on. Were the motion uniform, the mass of water passing would be 1005 A. On acceleration supervening over the whole length the velocity would vary from one end of the channel to the other; the mass, as Guglielmini says, would nttenuate, its velocity being mucb greater at the lower end than at the upper end; and, consequently, the mass on each section being the same, the area would also vary. Now this is precisely what distinguishes permanent motion from uniform motion, and no finite length can be assigned in whicb the character of the motion is not entirely changed. It is only by a mathematical fiction that any formula can be obtained for uniform motion; by assuming, that is, that tbe lengtb l is infinitely small, so mucb so as to be on the point of vanishing into a plane. A similar error is caused by taking a finite period of time, as a second, in whicb to consider the accelerating force as acting. This will in some measure be corrected by taking on the principle of virtual velocities the same time, to consider the retarding force; and then wben expressions are deduced for them by fixing the ideas with a definite, though small time, the two expressions can be equated by supposing the time to diminisb indefinitely, until on the point of vanishing the two sets of forces coalesce, and an expression is obtained for uniform motion.

The length l then, that can be admitted, is infinitely smaller than the length of mass that passes througb it; and this length, whicb may be represented by the mean velocity, is all that can be admitted into the equation.

304. We have yet to learn if any explanation can be found for the expression A D, as representing the accelerating force. Two or three extracts may throw some ligbt upon this.

305. Dubuat (p. p. 41-42) says (1) « If a globule is engaged between two other contiguous and motionless globules, the effort necessary to disengage it will be as much greater as it is desired to move it quicker; but if this globule is forced to move upon a pile of similar globules, *each resistance being as the velocities*, and the number of the particles being in the same ratio, it follows that *the total resistance should increase as the square of the velocities* ».

306. Contrast this statement with that of D.r Moseley (2); in his paper on the steady flow of liquid. He calls K the common area of surface of tbe planes of the fluid in contact, V the uniform velocity of the one filim over the other, nnd says « To determine tbe work lost per unit of time by the impact of the molecules of the moving filin over that of the fixed one, let it be observed that the loss is *proportional to the number of such impacts* per unit of time, and the work lost on eacb impact, and that the last is measured by half the *vis viva* lost in each impact. But the number of impacts per unit of time is proportional to K V; *and the vis viva lost in the impact of one molecule on another at rest is proportional to the square of the velocity of the impinging molecule* ».

(1) Principes de Hydraulique.
(2) Philosophical Magazine, July 1872, page. 53.

This is a complete statement of the principle of Leibnitz as generally received in its application to liquids. It is clear that the force lost by the one film is proportional to $K\ V^3$, or the number of particles into the square of the velocity of each.

307. On Dubuats statement the same would be expressed by $K\ V^2$; $K\ V$ being the number, V the velocity of each.

308. Neither of these statements would, however, acconnt for the expression A D as representing the accelerating force in the section of area A. The velocity of the mass is V. The velocity due to the acceleration according to Dubuat is $D^{\frac{1}{2}}$. On Moseley's statement the result would be A V D, on Dnbuat's $A\ V\ D^{\frac{1}{4}}$. The result as given must be left to the advocates of Dubuats theory to explain.

309. In order to carry on the comparison between the two theories of the flow of water the missing term will be introduced; and we have on Torricelli's principle the acceleration shewn as A V D P; on Dubuat's as A V D.

310. Dubuat's general formula being (See ante)

$$R^m D^n = f v,$$

or:

$$A^m D^n = \chi^m f v \text{ since } R = \frac{A}{\chi}$$

where χ is the perimeter, or more simply

$$A D = \chi f v$$

Here the accelerating force on the one side is represented by an area and the square of a velocity and on the other the retarding force is given by a line — the perimeter — and a function of the velocity. If there was difficulty in understanding the first, what is to be said of the second? There are two sets of magnitude contrasted. The velocities are of like kind and comparable with each ether, and with each other only. The perimeter is left to be compared with the area. In ordinary geometry this cannot be done. Can it be done in mechanics? The answer to this also must be left to others to answer.

311. That Dubuat, Prony etc. have really intended to represent the living force of the resistance by a function of the perimeter and the velocity may be easily gathered from their works; and later hydraulicians have continned the same anomaly. Thus Prony says: « The retarding forces that have place at the liquid couch adherent to the walls, and without which the uniformity of movement can not be established, are composed of those due to the cohesion of the fluid molecules between themselves, and to other forces peculiar to the limit of the system; further, these forces are proportional to the surface upon which they have place, and ought to be also a certain function of the velocity against this surface; but it has been seen that the mean velocity is a function of the molecules that move near the walls, or of the velocity of the bottom;

in supposing then the resistance to which the uniformity is due, proportional to the product of the surface of the wall by a function of the mean velocity (a function to be determined) the diverse elements of which the expression should naturally he composed can be made to enter into it ».

312. It is after this statement that he expressed a suspicion of a lacune in the formula. The lacune he felt is, the leaving ont of the entire mass of the matter, from which the resistance proceeds. The retarding force is not a function of the mean velocity, nor of the top velocity, nor of the bottom, nor can it be expressed by any of those in conjunction with the perimeter only. It is a force, and must necessarily have stated out the mass of matter concerned, and the square of the velocity, on Leibnitz's principle.

It is proportional to the whole force in the section, and the proportion is determined by the extent and nature of the perimeter.

313. That the mass must enter into the expression may be gathered from both Dubuat and Prony. An exactly similar extract to those now about to be given, has already been given from Guglielmini, so that the state ments are not peculiar to either theory.

314. Dubuat says: « If the water did not suffer any resistance from the part of the bed in which it rolls, if the attraction of the walls of its bed were nothing, if in fine its fluidity were perfect, its accelerating force, that is to say its relative weight would continually accelerate its movement and would precipitate its course, in the manner of heavy bodies which fall freely; that is to say, it would acquire without ceasing new degrees of velocity. *Thus the friction against the bodies, which by effort of the viscosity, is communicated to the whole mass,* and even the adherence that the molecules have between themselves and the walls are causes of resistance which are relative to the velocities so that the resistances augment in the measure that the velocities become greater. »

Nothing can be clearer than this, which is in a great measure a reproduction of Guglielmini, retaining his peculiar statement about a perfect fluid.

315. Prony says « We may, after these immediate results of experience, have a general idea of the accelerating and retarding forces which modify the action of the weight. Let us take, for simplicity, the case of a cylindrical pipe *and consider the total mass it contains,* as divided into two parts: the one central of cylindrical form, having for axis the axis of the pipe, and radius varying from zero up to D (D being the diameter of the pipe); the other mass being comprised between that of which we speak and the wall: the mean velocity of the central mass is greator than that of the mass that envelopes it; thus, in virtue of cohesion alone, a reciprocal action and reaction must have place between these two masses, the first of which tends to augment the mean velocity of the second, which, consequently produces the contrary effect npon the first. Similar considerations are applicable to an indefinite number of couches or cylindrical envelopes of which we can imagine the fluid mass to be composed. Each of these couches tends to accelerate that with which it is in contact on the side of the wall, and to retard that to which it is contiguous on the side of

the axis or the centre »... « the adherence which has lace step by step, from the central filet up to the immoveable couch fixed to the wall, is the sole cause assignable for the destruction of the accelerating force impressed by the weight on the interior mass. »

316. The whole mass is thus held back by the resistance at the wall, and the force impressed npon it, or in other words the work done upon the mass, is a function of the mass and the square of the velocity, that is, on Leibnitz's principle, of the whole living force, and is a proportional part of this; the proportion being determined by the extent, and, as later writers, M. M. Darcy and Bazin have fully proved, contrary to another of Dubuats theories, of its roughness or transverse rugosity.

317. The mass of liquid passing through the section of area A of the channel, with a mean velocity V, will as before be A V; and the total force if all the particles moved with the same velocity, which may for the present be assumed, would be $A\ V \times V^2$ or $A\ V^3$. The retarding force being determined by the extent of perimeter χ and its rugosity ρ, it may be expressed as a funcion of $\rho\ \chi\ A\ V^3$, or $f\ \rho\ \chi\ A\ V^3$.

318. This is true of a unit of width; but it immediately becomes obvious on balancing the forces, especially with reference to an indefinitely wide section, that if the resistance increased indefinitely with the perimeter and the mass, while the acceleration increased only as the mass, the velocities on both sides remaining constant, no eqnation could result. It becomes therefore necessary to introduce the width of the section as a factor on the acceleration side, as it is clear the force of acceleration increases with the width of the section, in the same degree that the retardation increases with the perimeter. In ordinary open channels, where the surface width W is the greatest width of section, this width may be taken to represent the increase. The accelerating force will then be proportional to A V P W D.

319. Conceive now the accelerating causes to have their action suspended for one second; and the retarding forces to be in full operation during that time. The velocity would diminish. Let it be V at the middle of the time. and the expression $f\ \rho\ \chi\ A\ V^3$ will still nearly represent the force of retardation for that second. The smaller the time is the more exactly will the accelerating and retarding forces be represented by the above expression; and as, when the motion is uniform, these forces are equal, by supposing the time to diminish indefinitely till on the point of vanishing, the proportion finally becomes an equality and

$$A\ V\ P\ W\ D = f \rho \chi A\ V^3 \qquad (1)$$

Dubuat's expression would be on the same reasoning

$$A\ V\ W\ D = f \rho \chi A\ V^3, \qquad (2)$$

or:

$$P\ W\ D = f \rho \chi V^2 \qquad (1)$$

and:

$$D = \frac{f \rho \chi V^2}{W} \tag{2}$$

320. The remarkable result given in equation (2) for Dubuats theory is deserving of notice. The meaning of the formula is, that if additional masses of water are thrown into the same channel, the slope remaining constant, there will be no increase of velocity. This is precisely the result that takes place with a solid, when placed on a plane inclined so as to allow the mass to slide down with uniform velocity. No addition to the mass, and no deduction from it (excepting it be reduced to very small grains) will cause any alteration in the velocity; as, by the known laws of friction, this increases with the pressure, that is with the mass, and the two increasing evenly there is no additional residual force to accelerate the velocity. It may be remembered that the difference between Guglielmini's theory and that of Dubuats is, that the former specially distinguishes the action of a liquid from that of a solid, asserting that while all the particles of the latter being bound together by cohesion, are compelled the move under an accelerating force with the same velocity, the liquid owing to the independence of its parts, has its particles moving with different velocities, these being due to the pressures upon them; and Dubuat says that though the mobility of the particles of a fluid permit it to pass down a channel when a solid cannot do so, the sameness of the efficient pressures, or forces acting on all the particles, compels them all to tend to travel with the same velocity; that is, in open channels, the liquid moves under the same laws as a solid. It is this idea that has been expressed in the above formula, and as Dubuat insists on this as the basis of his theory, the formula when carried through on acknowledged principles of mechanics, returns the same idea in an unexpected form, and is itself an entire refutation of the theory. For the commonest experience shows that on increasing the mass of a liquid the velocity also increases. There is therefore no use in carrying on this second formula any longer; the one deduced from Torricellis principle will henceforth be attended to.

321. We have now to examine what function of the whole force and perimeter, the resistance is. Canon Moseley gives a very clear account of the whole forces operating in the flow of a liquid through a pipe. He says: « Let the steady flow of a liquid in a horizontal circular pipe of uniform dimensions and roughness of surface be supposed to be maintained by the pressure of the liquid in a reservoir whose surface is always on the same level;

U = work done per unit of time on the liquid which enters the pipe by the pressure of that in the reservoir.

U_1 = work carried away per unit of time by the liquid which flows away from the extremity of the slope.

U_2 = work expended on the various resistances which are opposed to the descent of the liquid in the reservoir and to its passage from the reservoir through its aperture into the pipe.

U_3 = work expended on the resistance of the internal surface of the pipe to the flow of the liquid along it.

U_4 = interval work of the resistance of the films to the flowing of each film over the surface of the next in succession; then by the principle of vertical velocities,

$$U = U_1 + U_2 + U_3 + U_4 \text{ ».}$$

322. In an open channel with uniform motion we have to deal only with the last two, the work done being in each case one half the living force.

323. The distinction between these two portions of work, viz the work done on the mass of the water in retarding it, and the work done in the flow of the films over each other, which is work lost to the system and converted into heat or other form of molecular force, is very clearly discriminated by Dubuat.

324. The extracts already given refer exclusively to the work done on the water; one will now be given to show his views upon the other part. He says (Principes p. 58). « The viscosity of the water, or the adherence that its particles have between them, occasions a very small, but finite, resistance, which is opposed to their separation; now, there can not be uniform movement in the water, unless the filets take different velocities, according as they are nearer to or further from the wall, which retards and renders uniform the movement of all the mass. This inequality of velocities cannot have place without a mutual separation of the contiguous parts. The viscosity, or, if you will, the force with which these parts draw each other, opposes this separation; it needs then that there be a part of the accelerating force destined continually to overcome this resistance ».

325. Canon Moseley's paper is devoted exclusively to the consideration of this part of the force, for which he deduces a very complicated expression. It is moreover based on Dubuat's theory, and is therefore unsuited for the present discussion. It is to be regretted that he did not devote his great powers of analysis to the elucidation of the other force, which he evidently regards under the same view as his predecessors, since he makes it disappear under a double differentiation, thus shewing that he considered the perimeter — the vanishing factor — as an integral part of the force.

In this paper the perimeter is viewed as entering only as a proportioning number, an abstract coefficient, which, though it may vary as well as the variables under differentiation, does not do so as a function of them, and is therefore unaffected by any differentiation.

326. If the two portions of the retarding force be represented by a and b respectively, the first being the work done on the liquid, the second the force lost to the system by the friction of the liquid films sliding over each other, the formula will become.

$$P\,W\,D = (a + b)\,\chi\,\rho\,V^2.$$

327. It now becomes necessary to state that the most difficult part of the subject is reached, a part so difficult that the highest efforts of the mathematical specialist may fail to satisfactorily acconnt for the results given by experience, and I would be only exercising a wise discretion to present the rest of the formula as empirical results; a course for which there is ample precedent, and to avoid attempting to explain either how it has been arrived at, or what theoretical notions may have suggested it.

328. If, however, it is kept in mind that the purpose of the present paper is not so much to establish a complete theoretical formula, as to show that it is possible to derive a formula from Torricelli's principle, which will, as well at least, as any of those before qnoted, adequately represent the results of experiment, any suggestion from theory may be availed of, and any failures that may now take place to account for the terms here after employed may be set down to want of knowledge of the subject, and not to the theory. Some of the parts have been directly suggested theoretically, others arrived at empirically. In no case can any thing like a mathematical domonstration be given, and the statement of the terms will be rather an attempt at untechnical description than conclusive argument.

328. The extracts from Guglielmini, Dubuat and Prony concerning the resistance diffused through the mass, on modifying what is said about a perfect fluid so as to include D.r Moseley's view, appear unexceptionable. We may then conclude with them that the resistance generated at the walls of the channels is communicated to every filet of the water, however remote, and holds it back in some degree; and that this in turn pulls on in exactly the some degree the filets nearer the wall. That degree depends directly on the amount of force in the filet, that is on the number of the particles, and the square of the velocity; and inversely as the distance from the wall. As each filet is situated at different distances from the various parts of the wall, and its pull may be supposed to be diffused over every part of the area, we can imagine the nearer filets to compensate in their action for the more remote, and the work done on the water to be equalised over the whole section. The resistance over the whole mass will then be directly proportional to the whole force residing in the mass, and the term a will remain unaltered.

330. M. Navier's term $\frac{d\,v}{d\,r}$, where $d\,v$ represents the indefinitely small difference of velocity between two nearly contiguous films at an indefinitely small distance $d\,r$ apart, is the expression from which can be found the force consumed in the friction caused by the sliding of the films over each other.

331. Newton shows (*Principia Book II Prop XXI*, p. 306) (1) that if bodies oscilating in a resisting medium describe arcs which increase or diminish in a given ratio, the differences of the arcs discribed increase or diminish in the same ratio. The reason given is: « Those differences arise out of the retardation

(1) Thompson's Edition.

of the pendulum through the resistance of the medium, and therefore that retardation is proportional to the whole resistance of the medium. »

332. In the same way the retardation of each liquid film arises from the resistance of its neighbours, and is proportional to it. The resistance is proportional to the force impressed, and therefore the work lost is proportional to the same force. It has been usual to consider the velocity $\frac{dv}{dr}$ as the same between each pair of films; under Torricelli's principle, it must be supposed to vary as the square root of the depth, since the force generated, in the liquid films varies as the depth. The integration mathematically of these differential forces offers difficulties at present insuperable. A rude approximation only can be attempted.

333. The first aid from theory is, that if $\frac{dv}{dr}$ be used at all in the representation of the force, it must enter in its second power, by Leibnitz' principle. The number of particles in the whole section is already given in the espression A V. The force lost must be a proportional part of the whole force of retardation $\chi \rho A V^2$, or A V being eliminated from both sides of the equation of $\chi \rho V^2$, and its proportion is determined by the expression $\left(\frac{dv}{dr}\right)^2$. That is, it is proportional to

$$\chi \rho \left(\frac{dv}{dr}\right)^2 V^2.$$

334. M. Darcy's expression:

$$\epsilon R^2 \left(\frac{dv}{dr}\right)^2 = \frac{ri}{2}$$

or

$$\epsilon R \left(\frac{dv}{dr}\right) = \sqrt{\frac{ri}{2}}$$

is a most important contribution to hydraulic science. He does not give the interpretation of the factor R. I would venture with diffidence to offer the following suggestion respecting it. The whole expression relates to circular pipes, and it affirms that the liquid film whose distance from the centre of the pipe is r, and thickness dr, has its differential velocity dv, inversely as the radius of the pipe R. That is if the mean velocity of the pipe remain the same as well as r, while R varies, dv increase as R diminishes; and *vice versa*.

335. Now Dubuat (p. 33) says that when the mass of liquid increases in a greater ratio than the perimeter, all other things remaining equal, the retarding force of the perimeter on each particle must diminish. Guglielmini has similar state ments, and the fact seems sufficiently obivous. The same law holds in channels of every shape, and M. Darcy'e expression for a film whose position is at a depth r, thickness dv, and width W would be.

$$\epsilon \left(\frac{dv}{dr}\right)^2 = \frac{WD}{2R^2}$$

6

or changing the constants:

$$\left(\frac{d\,v}{d\,r}\right)^2 = \frac{\beta\,W\,D}{R^2}$$

By substitution:

$$\chi\,\rho\left(\frac{d\,v}{d\,r}\right)^2 V^2$$

becomes:

$$\chi\,\rho\,\frac{\beta\,W\,D}{R^2}\,V^2$$

336. We have now to consider that R is really $\frac{A}{\chi}$, and while this representst he average distance of the liquid films from the wall, it suggests another, which is found empirically necessary. Suppose the degreo of rugosity of the walls to be increased, what effects would follow? From the experiments made M. M. Darcy and Bazin it is clear that the whole mass of water would be retarded, and the velocity V would diminish. But as the force of acceleration would remain the same, the force exerted by each film would remain unaltered and therefore the differential velocity $\left(\frac{d\,v}{d\,r}\right)$ would be unchanged. If the accelerating force were then increased so as to restore the original mean velocity, an increased reciprocal action would take placo between the fluid films, each exerting a greater accelerating or retarding force upon its neighbour. The differential velocity would increase also. This double action may be represented in the above expression by attaching the coefficent of rugosity to the perimeter, so that:

$$\frac{1}{\frac{A}{\chi\,\rho}} = \frac{\rho}{R}$$

and the last expression found becomes:

$$\chi\,\rho\,\frac{\beta\,W\,\rho^2\,D}{R^2}$$

and the former equation:

$$P\,W\,D = \left(a + \frac{\beta\,W\,\rho^2\,D}{W^2}\right)\chi\,\rho\,V^2$$

337. Here comes a total break down in the attempt to carry on any theoretical explanation of the terms. Instead of the equation just given it has been found empirically necessary to substitute:

$$P\,W\,D = \left(a + \frac{\beta\,W\,\rho^2}{R^2\,D^{1/2}}\right)\chi\,\rho\,V^2$$

an equation which very curiously satisfies widely differing cases. It was found necessary to make the change, which was suggested by M. Bazin's second

formula, the one he afterwards abandoned. It has the effect of connecting together, with the same constants, channels of different slopss, and very nearly enables the Irrawaddi, the largest river yet accurately gauged, to have its discharge represented by the same figures as M. Bazins' experimental channels. α and β are two constants determined smpirically; ρ is the coefficient of rugosity, and if the smoothest of M. Bazin's channels — the one in cement — be taken to have a coefficient of $\rho = 1$; the formula will apply to the others by varying ρ from 1 to higher numbers according to the nature of the walls. M. Bazin's own formula and experiments are convenient for instituting a comparison. Before doing this, a very important question raised by M. Darcy (Recherches, pages 176-180) must be considered.

338. It is this. While the mass of liquid passing through a section A with mean velocity V, is correctly represented by A V, the force generated when the particles have unequal velocities, is not given by A V^2, and can only be shown by the sum of all the particles each multiplied by the square of its own velocity. Thus if the area were divided into three parts, such that equal masses passed through these parts in the same time, the velocities being 3, 4 and 5 the total force would be proportional to $\frac{9+16+25}{3} = 16,66$, while if all the parts moved with the same velocity 4, it would be proportional to 16. And if the velocity of the equal masses were as 2, 4 and 6, the total force would be as $\frac{4+16+36}{3}$, or as 18,6.

339. Darcy says « General Poncelet has demonstrated that this ratio is always greater than unity ». He then finds expressions for cases given. The question now arises, how does this fact affect the equation of uniform motion? First, it is obvious that the disturbing cause will operate on both sides of the equation, and since the mass is the same, the number of particles affected by the force in each part will be the same. The accelerating force follows the laws of its two causes for each filet; that is, it varies as the depth for each, and as the inclination for all. If the velocities of the filets, which give the mass affected, varied according to known law, one expression might be found for the summation of the force developed; but, as will be hereafter shewn, the relative velocities vary with every change of condition affecting the mass, and it is only in regular areas in invariable conditions that such a summation can be attempted. This also affects the summation of the retarding forces. But if it is considered that the retarding force in each filet is equal to the accelerating force developed, when the motion is perfectly uniform, it would appear that the equality would still subsist over the whole mass and each force, whether accelerating or retarding, bear a constant proportion in all conditions to the force represented by taking the square of the mean velocity as the multiplier. The whole subject is, however, so difficult that I only venture to throw out these remarks as suggestions.

It is a matter which falls exclusively within the province of the mathemat-

icean, and until a complete investigation of the problem in its generality is given, we must be content with approximate results.

340. Turning now to the ordinary form of the formula professedly founded on Dubuat's theory, an idea may be formed (supposing the earlier stages of the equation given in the paper to have been properly dedneed) of how those formulae have so efficiently performed their work. The lacune or lacunes of M. De Prony have been shown to be due to:

1.° Leaving out the factor P for the depth of the centre of pressure.

2.° Leaving ont the factor V on the acceleration side of the equation.

3.° Leaving out the double factor A V for the mass on the retardation side.

341. There is thus left the perimeter on the one side against the area on the other; and the expression $\frac{A}{\chi} = R$ results to fill the vacuity caused by the absence of P on the acceleration side. It has not filled its place unworthily; especially in the kind of channels in which the experiments made on the flow of water have been conducted. How remarkably it does so in some cases may be seen by comparing the values of the two expressions $\frac{P\,W\,D}{\chi\,V^2}$ and $\frac{R\,D}{V^2}$ for Bazins' rectangular channels. The former is just $^2/_3$ of the latter. On a large scale, at a section of the Irrawaddy at Saiktha where the river is over 5000 feet wide and 77 feet deep at high flood, P and R continue nearly alike. The gauge reads 40 feet at the highest known flood, that of 1871. Taking readings nearly 6 feet apart P and R are:

Gauge	36, 00	29, 75	24, 08	17, 75	12, 08	6, 00	1, 95
R	43, 6	38, 00	32, 3	26, 4	21, 3	20, 2	19, 3
P	35, 675	32, 40	28, 95	25, 8	23, 1	20, 15	18, 2

342. The resemblance is curious, considering the diversity of the methods by which the results are obtained. There are cases, however, in which the resemblance ceases, as in the pipes discussed early in this paper; and there is a want of elasticily or adaptability in R, which makes it unsuited for channels of irregular shape; whereas P can always be obtained with something like mathematical exactness, and does exactly represent its proper share of the force generated. The remarkable results that have been obtained from the formulae supposed to be derived from Dubuat's principle depend solely on this resemblance between the two factors; and the whole of those formulae are much more intimately connected with Torricelli's principle than with Dubuats; with which, indeed, they have no theoretical relation whatever.

343. The almost coincidence between the valnes of the expression $\frac{P\,W\,D}{\chi\,V^2}$ and $\frac{R\,D}{V^2}$ in the present formula and M. Bazin's enables an immediate comparison to be made. M. Bazin has plotted five series of observations on one sheet. These

channels differ only in the degree of rugosity of their walls; being in fact the same channel with walls of cement, boards, brick, small gravel and large gravel successively employed. He finds it necessary to give for these, five different sets of values for the constants α and β, which have no apparent connection. His diagram has been copied out from his work, and is given with a plate opposite to it showing these observations plotted in the same manner, with the same vertical scale, bnt a greater horizontal one, from the new formula (1). It will been seen that the same values answer for the constants α and β; and by changing the coefficient of rugosity according to the nature of the wall of the channel, the one equation serves.

344. Further, by introducing the square root of the declination into the denominator of β, the formula becomes capable of representing channels of different declinations with the same figures, at least approximately. No theoretical explanation can at present be offered for this; though one seems possible. The term β was taken from M. Darcy expression (or rather it was derived empirically and found to resemble that) which was deduced for pipes compelled to flow with full section under all conditions; whereas a change of conditions, and especially a change of declination brings with it in open channels change in the area occupied by the flowing water. Empirically, the factor $D^{\frac{1}{2}}$ satisfies the results of experiments nearly, when bronght in as above stated. It does not do it exactly. M. Bazin took some series of experiments on channels with the same kind of walls but of different slopes. The five series given by him in the diagram were all on the declination 0,0049. Two series, in boards, had declinations of 0,00824 and 0,00208 respectively, and these have been plotted by the new formula in place of bis series N. 7, of declination 0,0049, the place of which they occupy. A slight divergence between them may be seen, especially towards the experiments with the smaller quantities of water. The divergence indicates that the water is affected in a less degree by the rugosity of tbe channel wben the slope is steeper than when it is small. The velocity is of course mnch increased in the former case. Now, it is a well known and generally received notion amongst physicists that the friction of bodies, whether fluid or solid, tends to diminish as the velocity increases. M. Dubuat (pag. 42) in continuation of an extract before given, speaking of the entanglement (engrenage) of the particles in each other, says: « Experience teaches us that the resistances do not exactly increase in this ratio, bnt that it is almost the same here as with solid bodies, where the engrenage diminishes as the velocity augments ». A slight alteration in the value of the coefficient of rugosity would pass the line of the equation through either series; and it is possible that the increase of the velocity, especially in the observations where the water was shallow, caused the rugosity of the walls to have less effect in retarding the flow of the liquid films over each other.

(1) The abscissa being $\frac{W\rho^2}{R^2 D^{\frac{1}{2}}}$ and the ordinates $\frac{P W D}{\chi V^2}$

345. A series of accurate gaugings of the Irrawaddi has been made on different sections during the past year. The object has been to find the amount of water lost between the head of the delta at Saiktha and a point about 110 miles lower down at Zaloen, in order to test the effect of the new embankment works on the river. The observations have been made on the plan used on the Mississippi, as described in the Report by Messrs Humphreys and Abbott. One or two modifications were introduced. The surface velocities were taken daily, generally sixty in number, the lower float being one metre below the surface. Ten series of sub-surface velocities were also taken daily, the lower parts increasing one metre in depth, from one metre until the bottom was touched. The mean of each of these was taken and divided by the surface velocity on that line. The result was used as a correction to be applied to all the surface velocities in that division of the river in which it fell. The section was divided into ten parts. These observations are still being continued at Saiktha, as it is desired to have a complete year's record.

346. The surface slope of the river is one of the most difficult points the practical hydraulic engineer has to deal with. The highest flood level of different points of the river have been accurately connected by levelling for over 110 miles, and the average slope found to be about six inches per mile. At Mynnoung 14 miles south of Saiktha, and about 200 miles from the sea, the highest flood mark is 76 feet above mean sea level, by the Grand Trignometrical Survey level. The range of the river from highest flood to low water is about 40 feet at Myanoung, thus reducing the fall to the sea at low water to about one half what it has at high. The range at different points varies. At Saiktha it is over 42 feet; at Henzahdah 74 miles south of Myanoung 36 feet; and at Zaloon 34,5 feet. This surface slope would then be altered from high to low water by this cause alone; but there are other causes of variation. Thus there are several basins (*bassi fondi*) in this part of the river with ridges forming lips at their lower side, where steamers drawing only 4 feet of water are frequently stranded. These ridges hold up the water for many miles when the river is low, and while the slope above them is then reduced, it is much steeper immediately below them. In addition, the river when in full flood takes a much shorter course than when in the dry weather channel, so that if even the difference of level between any two points of the surface were the same in the different stages, the lengthening out of the course would considerably alter the surface declination. Now thè declination can only be approximated to by a series of averages. A short line of levelling along the bank, however accurately taken, will not give the fall on the thread of the current. This may be passing from one side to the other, or if it is parallel to the bank the local peculiarities of the channel may back up the water in some places, or give it a more than average fall in others. Thus the low water surface of the Irrawaddi has been found to vary from 2 to 4 inches per mile, while in some parts it is more. This difficulty would subsist if the surface were always a plane, varying regularly; but, in truth, it varies with every undulation of flood, and with the rise

or fall of the surface the declination may vary several inches. It is almost impossible to obtain an accurate record of each variation for the different discharges, and thus a serious disturbing element is introduced. We know, however, that if the surface were a plane the slope would vary with some regularity within certain limits. Thus at Saiktha at the very highest flood (gauge reading 40) the general slope on 10 miles was found to be 6, 68 inches per mile. At a gauge reading of 36, it was about 6 inches; and at the low water it is between 3 and 4 inches. By taking discharges of the river when it is above the low water channel, say between 14 and 36 feet on the gauge, we can have a set of data to test the formula by. The slope, which is not accurately known for the various stages, is taken to vary regularly from 6 to 5 inches per mile between 36 and 12 feet.

347. The centres of pressure have been calculated out for every 2 feet of the gauge from 40 feet downwards. This was done by the ordinary formula:

$$P = \frac{\int x^2 y \, dx}{\int x \, y \, dx}$$

where x is the depth, dx was taken as 2 feet, and y being tho width of the river at each depth x, was measured on an enlarged section. An attempt was mado to obtain an integrometre Deprez described in the Annales des Ponts et Chausses for 1871, but it was stated to exist only in theory. This instrument will be most useful in finding the centres of pressure, if it performs all that is promised; and it is to be desired that some of the instruments be tested, as the work of calculating out many river sections is very tedious.

348. A list of 14 discharges of the Irrawaddi is given with the measurements and calculations in metric measure, so as to place the result on the same diagram with the experimental channels. The abscisse are as before $\frac{W \rho^2}{R^2 D^{\frac{1}{2}}}$, and the ordinates are $\frac{P\,W\,D}{x\,V^2}$. A line drawn on the constants of the experimental channels, with coefficient of rugosity $\rho = 2,23$, is passed through the series. It will be seen that the low water discharges, and those with a falling gauge stand mostly above tho line; and the others below it. If the slopes were adjusted to the rise or fall of the gauge, and a total change of from 6 to 4 $^1/_2$ inches instead of from 6 to 5, were made in the slope, which is warranted by the levels that have been taken, the observation would nearly fall on the line. But as the present purpose is not to obtain a final formula with completo constants, the result is given with the discrepancies shewn. Another line with altered constants passes through the series much more nearly; and it is probable that when the whole of the data of the Irrawaddi have been obtained, and reduced; constants differing somewhat from those of small channels must be used. It can hardly be expected that a very large river flowing in tropical latitudes, with water often saturated with silt, can be represented correctly in the formula by figures taken

from channels two metres in width, flowing with clear water in a temperate zone. The nearness with which the formula has given the results is a matter of surprise.

349. Sufficient has now been given to show the grounds on which it is sought to reintroduce Torricelli's principle as the true one on which to base theories respecting the flow of water in open channels. The case against Dubuats principle has been made out exclusively from writers who are presumedly in favor of it; and as far as it was possible to do eo, the ideas necessary to the support of Torricelli's principle have been taken from the same writers. No new ideas have been introduced; and, it is thought, only acknowledged principles of mechanics have been used.

Henzahdah, the 31 May 1873.

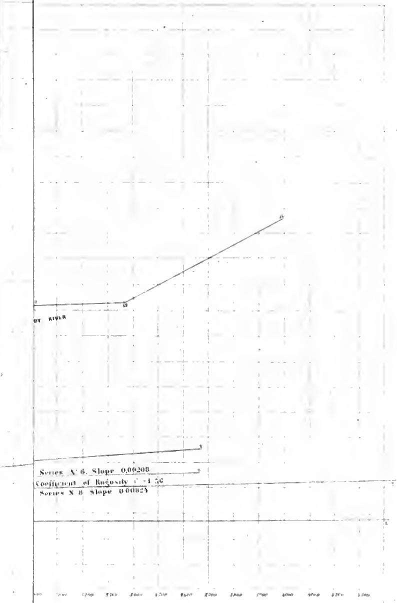
Robert Gordon

Executive Engineer
Irrawaddi Embankement Works and Delta Survey.
Myanoung Division
Public Works Department, India.

Numero (7)	Debit Par	Profondeur		Perimètre	Vitesse			Valeur		W
1	0, 100	0, 0468	0, 0931	2, 0836	1, 074	0, 0447		0, 000319	0, 000212	10072
2	0, 203	0, 0757	0, 1506	2, 1414	1, 348	0, 0703		0, 000319	0, 000212	4436
3	0, 307	0, 0968	0, 1926	2, 1836	1, 504	0, 0882		0, 000286	0, 000190	2818
4	0, 411	0, 1163	0, 2314	2, 2226	1, 776	0, 1041		0, 000272	0, 000182	2023
5	0, 515	0, 1361	0, 2708	2, 2622	1, 902	0, 1197		0, 000273	0, 000182	1530
6	0, 618	0, 1513	0, 3011	2, 2926	2, 053	0, 1313		0, 000257	0, 000172	1272
7	0, 721	0, 1657	0, 3297	2, 3214	2, 186	0, 1420		0, 000245	0, 000164	1087
8	0, 824	0, 1826	0, 3634	2, 3552	2, 268	0, 1543		0, 000247	0, 000164	921
9	0, 929	0, 1976	0, 3932	2, 3852	2, 357	0, 1649		0, 000244	0, 000162	806
10	1, 030	0, 2115	0, 4209	2, 4130	2, 417	0, 1744		0, 000240	0, 000160	722
11	1, 133	0, 2261	0, 4499	2, 4422	2, 518	0, 1842		0, 000239	0, 000160	646
12	1, 236	0, 2378	0, 4732	2, 4656	2, 612	0, 1919		0, 000232	0, 000154	602

Data selected from the Irrawaddie Observations.

Numeros	Data	ENGLISH MEASURES								METRIC MEASURES						
		Gauge Reading	Daily Rise or Fall	Surface Declination D	Area A	Discharge Q	Mean Velocity V	Wetted Perimeter χ	Surface Width W	Wetted Perimeter χ	Surface Width W	Mean Radius R	Centre of Pressure P	Mean Velocity V	$\frac{P\ W\ D}{\gamma\ V^2}$	$\frac{W}{R^{0} D^{\frac{1}{2}}}$
	1872	feet		feet												
1	Aug.30	36, 00	0	0, 0000947	223516	1442007	6, 451	5220	5020	1591	1530	13, 047	11, 008	1, 965	0, 000262	923
2	» 23	33, 75	R 0, 25	0, 0000933	212403	1212190	5, 706	5215	5008	1589	1526	12, 410	10, 428	1, 739	0, 000309	1026
3	Oct. 18	32, 25	R 1, 09	0, 0000925	204988	1148790	5, 604	5212	5002	1588	1524	11, 984	10, 197	1, 708	0, 000309	1103
4	Sep. 9	31, 83	F 0, 50	0, 0000925	202921	1098288	5, 365	5208	5000	1588	1523	11, 872	10, 117	1, 635	0, 000334	1124
5	» 29	29, 75	R 1, 00	0, 0000918	193083	985959	5, 106	5205	4992	1585	1521	11, 303	9, 872	1, 556	0, 000354	1242
6	» 21	26, 58	R 0, 92	0, 0000911	177123	871823	4, 922	5196	4990	1583	1520	10, 386	9, 217	1, 500	0, 000346	1476
7	Nov. 4	24, 08	R 1, 00	0, 0000905	164633	757132	4, 599	5191	4975	1582	1516	9, 663	8, 821	1, 401	0, 000378	1707
8	Oct. 30	23, 91	F 1, 25	0, 0000905	163313	712125	4, 360	5190	4974	1581	1510	9, 588	8, 792	1, 329	0, 000413	1733
9	Nov. 2	22, 08	F 0, 25	0, 0000898	154750	667268	4, 228	5185	4968	1580	1514	9, 094	8, 532	1, 288	0, 000426	1932
10	» 8	22, 08	F 1, 08	0, 0000898	154750	627925	4, 057	5185	4968	1580	1514	9, 094	7, 861	1, 236	0, 000438	1932
11	» 12	17, 75	F 1, 17	0, 0000884	133363	483477	3, 626	5160	4942	1572	1506	7, 875	7, 589	1, 105	0, 000509	2582
12	» 14	15, 75	F 1, 00	0, 0000887	123483	416139	3, 370	5150	4928	1569	1502	7, 034	7, 589	1, 027	0, 000560	3012
13	» 16	14, 33	F 0, 75	0, 0000871	116649	370072	3, 258	5140	4915	1566	1498	6, 915	7, 359	0, 993	0, 000567	3356
14	» 21	12, 08	F 0, 42	0, 0000864	105353	307256	2, 767	5020	4770	1530	1460	6, 393	7, 039	0, 843	0, 000731	3995


RIVER
Series N° 6. Slope 0.00208
Coefficient of Rugosity
Series N 8 Slope 0.00824